

OUR BOOK SHELF

Marvels of Pond-Life: or a Year's Microscopic Recreations among the Polyyps, Infusoria, Rotifers, Water-Bears, and Polyzoa. By Henry J. Slack, F.G.S., &c. Second Edition. (London: Groombridge and Sons.)

THIS little volume is already so well and favourably known to microscopists that any formal notice or commendation is scarcely necessary. Professing only to be a first book on "Pond-Life," it does not attempt more than to guide the young student in searching after, collecting, and examining the various animal organisms which inhabit fresh water. The division into months indicates that it is also popular rather than abstruse, and the number of species mentioned or figured is very limited. There appears to be no good reason why the present edition should not have made an advance beyond its predecessor, and given us an additional chapter or two on the construction and management of small aquaria at home, adapted especially and entirely to minute pond-life, by means of which metropolitan students might continue the study when unable to go to the ponds; and also on those artificial ponds for the evolution of Infusoria, so much alluded to of late, infusions of organic substances. Keeping in view the simple pretensions and elementary character of this volume, it fully answers the design of its author, and we are glad to announce the appearance of a second edition.

Physikalisches Repetitorium, &c., &c. Von Dr. Ferdinand Bothe. Second Edition, revised and enlarged. (Brunswick: Vieweg, 1871.)

A BRIEF enumeration of the more prominent facts and formulæ of physics; carefully divided into subjects, and with occasional dates and names of inventors or discoverers. We conceive that to make an excellent work of this kind (if such a thing be at all desirable), all that is necessary, is to take a really good treatise on natural philosophy and construct something between an Index to, and an Abstract of, its contents. It seems probable that some such process has been employed by Dr. Bothe; but either he cannot have used a trustworthy book for analysis, or his analysis is not a faithful one. In fact, if we look on it seriously, a more painful volume we have not often met with; nor a more amusing one, if we could fancy its blunders intended to amuse. We simply open its pages at hazard, and make a few pickings:—

"64. The density and resilience (*Spannkraft*) increase in proportion to the pressure, the volume is inversely as the pressure, and *vice versa*—Boyle's or Mariotte's Law, 1679." James Bernoulli was a contemporary of these men, and says in his work, "*De Gravitate Aetheris*," "*Veritas utriusque hujus regulæ manifesta fit duobus curiosis experimentis ab Illustr. Dn. Boylio hanc in rem factis, quæ videtis [sic] in Tractatu ejus contra Linum.*" The date of this tract of Boyle's is 1662, and it is to be observed that Bernoulli does not mention Mariotte at all. We notice, in passing, that Young's name is not mentioned under Capillarity, and we arrive at the following curiosity:—"140. Unit of momentum or of work (*Arbeit*) is the force (*Kraft*) which can in one second communicate to unit of weight a velocity of unit of length. (Its metrical measure is the kilogramme-mètre; in Prussia, England, &c., the foot-pound." But we beg Dr. Bothe's pardon. We had no right to render *Arbeit* by "work," which is its usual equivalent in scientific books; for looking back we find:—"129. The product of the weight of a body into its velocity is called Momentum, and also *Arbeit*!" It is scarcely possible to conceive a more hopeless jumble of essentially different things than these sentences exhibit. The Heliotrope is (468) ascribed to Gauss, 1830 (?). Did not Drummond use it in 1826? 471 gives Bunsen and Kirchhoff the credit of the spectro-scope, with its collimator, &c. What of Swan? As to the

equality of absorption and radiation, Angström is given without date, Stokes and Balfour Stewart not mentioned. "472. The planets and comets (!) send back only the rays which the sun has sent to them." 484. In the enumeration of the earliest attempts to produce photographic impressions, there is no mention of Wedgwood, &c. 558. No mention is made of Northmore, whose long priority in the liquefaction of gases was insisted on by Faraday. 592. The old story of Mayer and the dynamical theory of heat. His date is given as 1842; Davy and Rumford (who did all that is referred to in the text more than forty years before) are not mentioned. Joule is coupled with Clausius, and the date 1853 is assigned to them! Of Carnot, Colding, Rankine, Thomson, &c., not a word. 598-600. The experimental laws of heat of combination are very imperfectly given, and, without any mention of Andrews and Hess, handed to Thomsen and Favre and Silbermann, with the date 1853! 666. The similarity of the order of bodies considered separately as conductors of heat and electricity is given to Wiedemann and Franz in the same prolific year. Surely Forbes pointed it out twenty years earlier! So far as we have seen, Sir W. Thomson, Clerk-Maxwell, &c., are not even named in the book.

If the reader remember that these are merely the things which have caught our eye in turning over the pages at random, he will not blame us for absolutely declining to examine the work more closely. A series of working tables is appended, but without very close examination we should hesitate to trust them, after what we have seen of the character of the book. That we have noticed it at all is due to the circumstance that some consolation is to be derived from the mere fact of its existence. We are all (in consequence, perhaps, of recent events) more or less imbued with the notion that Germany (Prussia especially) is rapidly taking the lead in matters of scientific education and investigation; and no doubt there is some truth in this. But the game is not lost, we are not yet passed in the race, and our old supremacy is quite within our reach even now, provided we make speedy and sufficient exertions to regain and maintain it. It will not drop into our mouths for a mere wish; but is it reasonable to wonder at the state of science in this country, where so few statesmen pay the least attention to it, when we find that even in enlightened Prussia, such a book as the above can be written by a recognised teacher, and published in a second edition by one of the highest firms in the world?

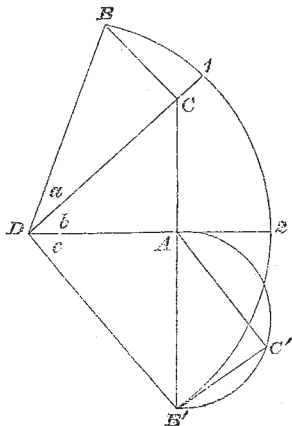
LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

Proof of Napier's Rules.

I AM greatly obliged to "J. J. W." for pointing out the objection of a want of generality in the construction of the figure contained in my former letter (in *NATURE*, No. 106), for the proof of Napier's Rules; which the more general construction now described by "J. J. W." most simply and most effectually removes. To illustrate his more perfect general construction with a figure—D is the centre, and B12B' a part of the circumference of a circular piece of cardboard, upon which the arcs B1, 12 are taken equal to the sides of the right-angled spherical triangle which it is required to represent. If we join DB, D1, D2, and draw BC, CA perpendicular to D1, D2, the latter perpendicular prolonged meeting the circle of the circumference in B', and join DB'; and on AB' as diameter describe the semicircle AC'B'; and with the centre A, and radius AC, another circle, meeting the semicircle in C', so that the straight line AC' is equal to AC; and join B'C'. Then it is easily shown that if AC CB are the two sides, AB' is the hypotenuse of a right-angled triangle, which, when the four triangles are closed together so as to form a solid figure, will coincide with the triangle AC'B'. As BC (or B'C') will then be perpendicular both to CD and to CA (or C'A), it will be perpendicular to the plane DCA; and the

arc B_1 , which is in the same plane with it, will be at right angles to the arc 12 . The third arc $2B'$ will therefore be the hypotenuse of a right-angled spherical triangle, of which B_1 , 12 , are the two sides. Calling these arcs or the angles of the faces represented by them, a, b, c , and the angles opposite to them in the spherical triangle, A, B, C , the proof of Napier's Rules, with this solid figure, proceeds by the same direct steps as those already described, with a special example of the figure in my former letter. As the construction there described is confined to the representation of a particular kind of right-angled spherical triangle, and is therefore inapplicable to illustrate the proof of



Napier's Rules experimentally in every given case, the general construction supplied by "J. J. W.," which is limited by no such restrictions, and which is at least equally convenient, will evidently serve more effectively the same practically useful and instructive purpose.

Instead of "accessible," as applied to the difficulties of the geometrical proofs produced by Mr. Cooley in his letter on "Elementary Geometry" (in *NATURE*, No. 103), which are indeed there obviously overcome, I would have used the word "surmountable" as more descriptive of geometrical difficulties, properly treated and discussed, had the word immediately presented itself to me; but having often found an easily executed model extremely useful and convenient in practical applications of Napier's Rules, with whose design, as a general resource to facilitate their study, I was not, however, so fully satisfied, I applied, perhaps unconsciously, to Mr. Cooley's demonstrations a term expressing strictly only the diffidence with which I ventured to present to readers of *NATURE* my own very imperfect geometrical contrivance. In thus making my difficulties accessible to "J. J. W.," I very gratefully acknowledge the assistance which I have derived from his remarks on my letter in *NATURE*, No. iii., and I cheerfully admit the merit and superiority of the general rule for constructing a proper model in cardboard, to illustrate the proofs of Napier's Rules, and to facilitate their study, which he has kindly consented to describe.

Newcastle-on-Tyne, Dec. 16

A. S. HERSCHEL

Alternation of Generations in Fungi

I AM sure that the Rev. M. J. Berkeley will exonerate me from any deliberate intention to misrepresent him; nor do I think that there is, after all, much difference of opinion between us regarding the present subject, unless, perhaps, that I am more sceptical. I alluded to the paper cited by him from the "Journal of the Horticultural Society," on propagation of bunt spores, and not to his communications on the hop or vine mildew. I was under the impression that he regarded the "four consecutive forms of reproductive cells in the bunt" as an instance of alternation of generations. On reference to the original paper, I find that he did not go so far then as to indicate four consecutive forms of reproductive cells; but that Tulasne followed on his track in 1854, and in 1857 Mr. Berkeley seemed to have accepted the results of Tulasne's observations, since, in his "Introduction," he gives figures at page 318, in the description of which the following phrases occur:—"spores of the second order," "spores of the third order," "spores of the fourth order." Here are the "four consecutive forms of reproductive cells" to which I alluded. At page 321 he writes concerning the bunt:

—"The spores, however, are not immediate means of propagation; they are, in fact, only a sort of prothallus, from which the mycelium grows, producing at the tips, or on lateral branchlets, bodies of various forms, which are themselves capable of germination, and immediately reproduce the species." The real issue between us seems to lie in the phrase, "alternation of generations." If the bunt spores, on germination, produce fusiform bodies, which, after conjugation, produce short cylindrical spores, and thus intermediate reproductive cells unlike the parent cell come between that and the ultimate reproduction of the species, I am induced to call it an "alternation of generations." It would be waste of time to discuss phrases, or I might take exception to the application of this phrase to the *Erysiphe*. The conidia and pycnidia of the hop mildew may be developed without sporangial conceptacles, and the parasite reproduced without sporangial fruit, but I cannot recognise alternation of generations in the reproduction of a species by means of conidia, stylospores, or sporidia, or by one of these alone. If such may be construed into an alternation of generations, it must be by permitting greater elasticity to the phrase. Conidia germinating and producing pycnidia, the stylospores of the pycnidia germinating and producing sporangial conceptacles, containing the sporidia which, upon germination, will produce the mycelium and conidia again, returning to the original form after two or three consecutive departures from it, appears to me a perfect type of alternation of generations. I fully admit that "if it is once established that a Puccinia produces an *Æcidium*, or an *Æcidium* a Puccinia, we should have a clear case, especially when the third form reverts to the first again." Without the slightest desire to "depreciate the labours of Oersted and De Bary," I cannot admit that they have established facts until their observations are confirmed, especially when there is an evident possibility of their having been deceived. I shall have no hesitation in accepting the facts when they are confirmed by independent and equally trustworthy observers, although I may be unable to account for some of the phenomena. At present I must confess that I am not so sanguine as Mr. Berkeley appears to be.

The correspondent signing himself "Mycelium" wishes to know if "the liability to produce parasitic fungi is communicated from the seed to the mature plant." In some instances we know such to be the case, in others perhaps only suspect it. The "bunt" is an instance, or why the steeping of seed corn? or how did the Rev. M. J. Berkeley succeed in producing bunted wheat plants from seed corn inoculated with bunt spores? Two or three years since I published particulars of a similar instance of celery seed and *Puccinia Apii*. It would be as rash to affirm that this is always the case as to deny its occurring at all.

M. C. COOKE

In Re Fungi

THE letters in your last two numbers have reminded me how ill this subject is studied by some botanists in this country. I will give two recent instances: 1. In the last number of the *Journal of Botany*, p. 383, it is positively stated that *Agaricus cartilagineus* (a rare and very critical species by the way) was determined by a growth which is there described a mere mass of mycelium. He must have been a bold man who ventured to name an agaric (above all things) from a mass of mycelium. 2. In the first number, October 1871, of the new edition of "Paxton's Botanical Dictionary"—"enlarged and revised"—under the article *Agaricus* there is to be found such a collection of obsolete names and absurd errors as to make the article simply ridiculous.

W. G. S.

Mr. Lowne and Darwinian Difficulties

MR. LOWNE (*NATURE*, December 7) sees no difficulty whatever in explaining by what natural process an insect with a suctional mouth is developed from one having the mandibular type of mouth, but still he does not explain. He affirms there is no doubt that "the pupa state is a modification (!) of the ordinary process of skin shedding," and that this is "proved" by so many facts that he cannot understand how it could be "denied," &c., but he does not prove it.

For aught I can tell, every internal tissue and every external scale of the butterfly may be represented in the larva; but I do not know and cannot prove that this is so, nor do I believe any one can prove it. That the changes which take place during the pupa state are very different from those that occur during any portion of the larva period, will be admitted by every one who